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A Case in which Alcohol was detected in the Brain—with Remarks.

By Samuel Lewis, M. D.

To the Editors of the Medical Examiner.

Gentlemen,—During my late residence, as house surgeon, in the Royal Infirmary, Edinburgh, the following case came under my care. It may, perhaps, be deemed of sufficient interest to deserve a place in your valuable journal.

Your obt. servt.,
Saml. Lewis, M. D.

Philadelphia, April 22d, 1842.

Walter Smith, ætatis 45, (vagrant.) This man was admitted into the Royal Infirmary on the evening of the 8th of June, 1840. He was brought in by the police, who stated that he had been found, about half an hour previously, at the foot of a flight of steps in one of the wynds, in the neighbourhood of the hospital. No further history of the case could then be obtained. On admission he was in a perfect state of coma, with laborious breathing, cold extremities, contracted pupils, and the pulse, at the wrist, barely perceptible. The countenance was pale and turgid; a good deal of frothy fluid was discharged from the mouth; and the *breath had a strong alcoholic odour*. No injury could be detected on any part of the body. An emetic was given immediately, and hot applications applied to the extremities. The emetic produced no effect; and before the stomach-pump could be introduced the patient died,—having lived about twenty minutes after admission.

On questioning his wife next day, I learned that he had left home on the previous morning quite well, on one of his usual begging expeditions; that he had, for a long time back, been in the daily habit of getting "free," i. e. drunk; and that lately, on one of these occasions, he had been so bad as to require the use of the stomach-pump.

Sectio Cadaveris thirty-six hours after death.

Head.—On removing the calvarium, the vessels on the surface of the brain were observed to be more than usually filled with dark blood; and there was an effusion of serum under the arachnoid sufficient to elevate that membrane above the surface of the convolutions, at the middle and posterior parts of the brain. The cerebral substance was firmer than natural, and presented an unusual number of bloody points when cut into. About an ounce of serum was found in the ventricles.

Chest.—The posterior part of the *left* lung was gorged with blood; and the bronchial tubes contained a very considerable quantity of a frothy fluid. The heart was of its normal size, but the right side, as well as the ascending and descending cavæ, were filled with dark fluid blood.

Abdomen.—The stomach contained eight ounces of fluid, which was

thought, by some present, to smell of malt liquor. The mucous membrane, throughout its whole extent, presented a healthy appearance. The liver and other abdominal viscera were free from disease. No alcoholic odour could be detected in the brain, or in any other part of the body.

In this case the patient, when brought into the hospital, seemed labouring under the effects of excessive intoxication, and was treated accordingly ; but neither the history of the case, nor the appearances after death, were sufficient to authorize a *decided* opinion in the affirmative. The alcoholic odour detected on his admission, and the account given by his wife of his fondness for strong drink, certainly favoured such a supposition, but were far from being conclusive evidence that death was caused by poisoning from alcohol.

As it was considered of some importance to ascertain, if possible, the cause of death ; and as it had lately been shown that alcohol could be easily detected, by chemical analysis, in the brains of animals, when poisoned by alcoholic liquors,* I determined to endeavour to settle the question in this way. I accordingly, in the afternoon of the same day, subjected about eight ounces of the brain to analysis, (in the manner to be mentioned more particularly presently,) having added previously a small quantity of water. After a drachm and a half of the liquid was distilled over in a test tube, the operation was discontinued, and an adequate quantity of sub-carbonate of potassa (well dried) introduced into the tube, which was then hermetically sealed, and the contents well shaken. The mixture was allowd to settle, and after a little while a stratum of a very mobile, oily-looking fluid was distinctly observed floating on the saturated solution of potassa, separated, however, from it by some ash-coloured flocculi. In order to prove that this supernatant stratum was alcohol, it was tested next morning in the presence of Drs. Reid and Low, both by flame and camphor. It burned readily with a blue flame, and dissolved camphor rapidly, leaving little or no doubt as to the nature of the fluid.

In making this analysis, (in which I was so fortunate as to obtain the assistance of Dr. Thomas Anderson, of Leith) the directions given by Dr. Percy in his Thesis, already referred to, were strictly observed. These directions I shall now give in Dr. Percy's own words ; and I do this the rather as the essay may not be easily obtained in this country, in consequence of its being chiefly published for private circulation.

"As the success of the operation," observes Dr. Percy, "in a great measure depends upon the manipulation, it is necessary to give a detailed description of the apparatus employed, and the precautions to be observed in conducting the process. Let us suppose, then, that it is required to examine some particular tissue, or, for example, the brain of a dog, in which we suspect alcohol. The entire brain, after being carefully removed, is cut into slices, which are transferred as quickly as possible into a small mattrass ; a quantity of water, sufficient to cover the whole organ thus sliced, being next poured into the vessel, a bent glass tube is adapted to the mattrass, and distillation commenced over an Argand lamp. The distillation may also be effected in the chloride of calcium bath without the addition of water to the matter subjected to analysis. As soon as ebullition begins, the operator must attentively watch the mattrass, and so regulate the heat as to prevent any portion of the

* Vide Dr. Percy's Prize Thesis "On the presence of alcohol in the brain." *Edinburgh*, 1839.

froth that is generated from passing into the recipient. When about 3ss is distilled over, the operation may be discontinued. The product is introduced into a small test tube, containing an adequate proportion of sub-carbonate of potassa. The tube is then hermetically sealed, and, after brisk agitation, is allowed to remain at rest. If alcohol be present in appreciable quantity, it will be found collected into a stratum at the surface of the saturated solution of potassa; from which, however, it will generally, if not always, be separated by some ash-coloured flocculi. On the contrary, if alcohol be not present in appreciable quantity, there will *generally* be no appearance of any supernatant stratum of liquid, although the flocculent matter may have accumulated at the surface as usual."

It now remains to describe the manipulation which is practised in investigating the properties of the supernatant stratum of liquid eliminated, as above, by the subcarbonate of potassa. The top of the tube being broken off, the fine extremity of a capillary pipette is carefully introduced just below the surface of the contained liquid, when, in obedience to the law of capillary attraction, a portion will rise in the pipette, which may afterwards be withdrawn; in this manner a sufficient quantity of the stratum may be readily obtained for examination. In order to prove whether it have the property of inflammability, the fine extremity of the pipette is to be placed near the edge of the flame of a lamp or candle, when the liquid, which is then to be gently propelled by blowing at the other extremity of the pipette, will, if alcohol, instantly take fire and burn with a blue flame. To make the application of the camphor test, a drop of the supernatant stratum is to be removed by means of the pipette, and deposited on a small fragment of camphor placed in the field of a simple microscope. If the liquid be alcohol, it will immediately be traversed in all directions by currents proceeding from the camphor, which will, in this case, be quickly dissolved; but the alcohol will rapidly evaporate, and leave a well defined patch of camphor, consisting of little masses mixed with plumose crystallization.

The above is, I believe, only the second case in which alcohol has been detected in the brain by chemical analysis. We have, however, many cases on record where it is *stated* to have have been found in the *fluid of the ventricles*, after death from poisoning by alcoholic liquors. A few of these may be cited. Dr. Cooke, in his treatise on nervous diseases, states, that he had been informed by Mr. Carlisle, "that a few years since a man was brought dead into the Westminster Hospital, who had just drunk a quart of gin for a wager. The evidences of death being quite conclusive, he was immediately examined, and within the lateral ventricles of the brain was found a considerable quantity of limpid fluid, distinctly impregnated with gin, both to the senses of smell and taste, and even to the test of inflammability. The liquid appeared to the senses of the examining students as strong as one-third gin to two-thirds water." In the 40th vol. of the Edinburgh Medical and Surgical Journal, other cases are recorded by Dr. Ogston, of Aberdeen, in his valuable paper "On the phenomena of the more advanced stages of Intoxication." We find, also, a case, bearing on the same point, in the 1st. vol. of the London Lancet for 1836–37, p. 271, in which, after the exhibition of half an ounce of sulphuric ether, "a strong smell of ether was perceived," on removing the skull cap. "Very moderate effusion in the lateral and fourth ventricles, which smelt strongly of ether, and a piece of lint soaked in in it, and held in the flame of a candle, was thought by the gentlemen present to blaze up rather more than a piece of lint dipped in water, the experi-

ment being made with water also at the same time." Lastly, Dr. Copland, speaking on this subject, observes, "Dr. Ogston confirms the testimony of Wepper, Voight, Carlisle, and others, as to the effused fluid being impregnated with alcohol. In describing the appearances in one of his cases, he states, that about four ounces of fluid were found in the ventricles, having all the physical qualities of alcohol, as proved by the united testimony of two other medical men, who saw the body opened, and examined the fluid."* (Dict. Pract. Med., part iii., Art. Drunkenness.)

These statements, at first sight, appear conclusive; but, on a closer examination, it will readily be perceived that they are not of so specific a character as not to admit of being questioned. We, accordingly, find Professor Christison making the following strong objections to them. "In former editions of this work," observes Dr. C., "I have ventured to question the accuracy of such statements, on the one hand, because in animals poisoned with alcohol introduced into the stomach, I never could perceive the smell in any other part of the body; and, on the other hand, because I have several times remarked, in the venous blood and brain of a fresh subject, a smell which a pre-possessed person might have confounded with that of alcohol, although no spirituous liquor had been taken before death. I must still repeat my doubts, chiefly, however, with the view of calling the attention of pathologists to the subject, and of expressing my surprise that those who imagine they have observed so very extraordinary a physiological fact as the transference of a spirituous fluid, not much diluted, from the stomach to the ventricles of the brain, should not have thought it indispensable to substantiate the fact in the only way in which it admits of being substantiated, namely, by chemical analysis. (Work on poisons, p. 853, 3d edition. Edinburgh.)

It was to meet these objections, particularly the last, that Dr. Percy undertook the investigation of the subject; and I think I am justified in stating that he has succeeded in proving, beyond the shadow of a doubt, that alcohol can be detected in the system after poisoning from alcoholic liquors. He found it not only in the brain, but also in the blood,† urine,‡ bile, and liver,—and he connects (perhaps justly) this latter fact with the frequency of diseased liver in habitual drunkards.

Dr. Percy's experiments were chiefly made on dogs, and the symptoms

* It is much to be regretted that Dr. Copland, after speaking so decidedly on the subject, has not informed us how the fluid was *proved* to be alcohol. Dr. Ogston, in his paper already alluded to, is altogether silent on this point, only stating that the effused fluid had "all the physical qualities of alcohol," but what these *physical qualities* were, or how ascertained, he does *not* mention.

† The credit of first detecting alcohol in the blood is, I believe, due to Majendie, (Dict. de Médécine, tome xii., p. 489.) Dr. Trotter, in his book on Drunkenness, urges the probability that "much of the liquor (alcoholic) enters the circulation, and gives there an additional stimulus." In a review of this work, in the Edinburgh Medical and Surgical Journal, the reviewer maintained, "that neither the blood, nor any of the excretions, when examined, give any indication of the presence of alcohol or intoxicating substance." (Vol. I., p. 57.)

‡ Müller, in his work on Physiology, enumerates alcohol as one of the substances which, when taken into the stomach, cannot be detected afterwards in the urine, (Baly's Translation, p. 589.) Berzelius, if I mistake not, has some where made a similar statement. Dr. Percy's experiment on this point was made in the presence of *Professor Traill.*

and post-mortem appearances are recorded with the greatest minuteness. He has, however, given *one* case in which he detected alcohol in the human brain, and we are happy to find that, in this case, "Dr. Christison confirmed the analysis." We may, therefore, expect, in the next edition of the Professor's work, a recantation of his previous opinion, and due credit given to Dr. Percy for "substantiating the fact in the only way in which it admits of being substantiated, namely, by chemical analysis." In none of these experiments was Dr. Percy able to obtain alcohol from the *fluid of the ventricles*, although a sufficient quantity was readily detected in the substance of the brain,—a result in direct variance from the statements of Drs. Cook, Ogston, &c. I conceive, however, that the experiments related under this head are not conclusive, as in no case was there a sufficient quantity of serum obtained to give the analysis a fair trial. This part of the subject, therefore, is still open for investigation. Should Dr. Percy's statement be confirmed by future experiments, it will present a very curious physiological fact, and one not easily explained. Can there be any affinity between *alcohol* and the *cerebral substance?* "We know (says a writer in the British and Foreign Medical Review) that medicinal substances have been detected in the blood, and that the action of many is more directed upon particular organs than others. How far this fact may be explained, on the supposition of a particular affinity of the medicine for the organ, is a subject well worthy of inquiry." This is a wide field for experimental research, and, if properly cultivated, cannot fail to give us many very important results.

There is, perhaps, no subject in physiology that has given rise to more controversy than the *modus operandi* of narcotic poisons; and they are few which are enveloped in greater obscurity. At present, no less than *three* theories exist on the subject.

First, That the effects of these poisons on the nervous system are caused solely by an impression upon the extremities of the nerves of the stomach, or other organ to which they may be applied.

Second, That the effects are produced by the poison entering the circulation, and making its impression upon the *extremities* of the nerves which are distributed to the coats of the blood-vessels.

Third, That all narcotic poisons (not even excepting prussic acid, whose action is almost instantaneous,) exert their deadly influence directly upon the central organ of the nervous system.

This is not the proper place to enter on the various arguments which have been brought forward in support of each of these doctrines. The discrepancy that exists among authors is merely mentioned to show the necessity for a further investigation of the subject. A patient and judicious perseverance in the path of experimental research may, ultimately, lead to a successful solution of some, at least, of the many difficulties with which it is at present beset; and the bringing to light of a single, and, perhaps, simple fact may at length disperse the darkness which surrounds this most interesting physiological question.

I think it may easily be shown that the *direct* modes in which alcohol produces its *narcotic* effects cannot be explained by an appeal to any *one* of the theories which I have mentioned. Such an enquiry, however, would extend this communication to too great a length, and would, moreover, necessarily lead us into a controversial discussion not adapted to the pages of this journal.

On the Treatment of Obstinate Constipation.

By GEORGE F. CARMICHAEL, M. D.

To the Editors of the Medical Examiner.

GENTLEMEN,—I forward the results of practice in one or two cases which have came under my care. If they are of any practical importance, and are in your estimation worthy of insertion in your valuable journal, you are at liberty to publish them.

Most respectfully, your obt. servt.,
GEO. F. CARMICHAEL.

Fredericksburg, Va., April 27th, 1842.

Captain C., a seafaring man, has had permanent stricture of the rectum for five or six years, to such a degree as to subject him to habitual difficulty in evacuating his bowels, and occasionally to complete obstruction and retention of the fæces. At the time I was called to see him he had had no evacuation from his bowels for five days, and at that time was suffering greatly from aggravated symptoms of constipation, viz., violent pain and enormous distension of the abdomen, sick stomach, constant vomiting, great thirst, sleeplessness, jactitation, inability to remain in a recumbent posture, oppressed and anxious countenance, &c. Previously to my visit he had taken numerous doses of purgative medicine, some of which were retained, and others rejected.

Prescription.—Bloodletting to ℥xx; warm bath; purgative draughts; enemata. No relief being induced by these means, and the symptoms increasing, I introduced the gum elastic stomach tube of a patent injecting apparatus, its whole length, (twenty-four inches, per anum,) into the bowels, and threw up gently and slowly about a quart of warm soap and water. This was retained for some minutes, and its reflux then permitted through the tube. Whenever the eyelets of the tube became obstructed by undissolved fæcal matter, they were cleared by injecting a little warm water through it. In this manner the accumulated fæces, (the cause of the symptoms,) dissolved in the soap and water, were removed in a few minutes, and the patient expressed himself entirely relieved and immediately sunk to sleep. No farther treatment was necessary.

In a second case the same proceeding procured immediate relief, after the patient had been subjected for a whole week,—he being in the greatest distress,—to every variety and mode of treatment which is usually followed in such attacks,—and when he was almost given up in despair. In the second case there was no stricture.

Cases of the above character are commonly regarded by practitioners of medicine and friends of patients with peculiar anxiety and solicitude; and as I feel assured that a most frequent cause of such cases is accumulation of hard fæcal matter in the large intestines, particularly in the ascending colon, insoluble by the secretions of the lining membrane of the bowels—incapable of being urged along the intestinal tube, either from its bulk or loss of peristaltic power from over distension—and inaccessible to common lavements—I can recommend the above mode of injection for solution and removal of fæcal obstructions of the bowels as satisfactory and safe, at least so far as my experience goes. I of course do not pretend to originality in the adoption of this plan, but merely testify to its sufficiency in time of need, when the usual means are defective.

ANALECTA.

Observations on Hydrated Peroxide of Iron, demonstrative of its decrease in power, as an antidote for Arsenious Acid, by age, and some hints on the method of preparing it. By WILLIAM PROCTER, JR.—From the April number of the American Journal of Pharmacy we extract the following very important observations on the Hydrated Peroxide of Iron, by Mr. Procter, an able correspondent of that excellent journal:—

Many months ago, (says Mr. P.) in preparing some tartrate of iron and potassa, not having quite enough of the recently precipitated oxide of iron, a portion of the hydrated oxide, which had been prepared nearly a year before as an antidote, and kept under water, was resorted to, but it was observed that the latter was dissolved very slowly by the tartaric acid, and the great difference in that respect from the recent was a matter of surprise, and suggested the idea that a like difficulty would occur in a still greater degree when arsenious acid was employed. Under the impression that, if several specimens of the hydrated peroxide of different ages, which had been kept under water, were subjected to trial, as to their activity in removing arsenic from solution, conclusions might be drawn which would settle the question definitely, a series of experiments were undertaken, and the results of these corroborate the suggestion that this substance gradually decreases in activity by age, notwithstanding it may be kept under a stratum of water. If this position is fairly established, as it is believed to be by the following observations, then it becomes a matter of some moment, both to the physician and the apothecary, to be acquainted with the fact and adopt a remedy.

Nine specimens of the hydrated peroxide of iron, all of which had been kept in the moist way under water, and most of them in bottles hermetically sealed, were treated in the following manner, viz.: The percentage of dry oxide which they severally contained having been ascertained, so much of each as is equal to thirty-six grains of the oxide, was placed in a vial, and three grains of arsenious acid, in solution, added, the mixture occasionally shaken, and tested to ascertain if the arsenic had been removed from the solution.

The result of these experiments showed a difference in the activity of the specimens quite apparent, the least active being on the side of age. But, independent of any influence that mere age may have in its deterioration, other circumstances most probably influence the powers of the preparation. It was invariably found, that those specimens which were most diluted, that is to say, which had the largest proportion of water admixed, all other circumstances being equal, were least active; whereas, those which preserved the state of *magma*, having little water mixed with them, were most active. By having much fluid associated with them, the particles of the hydrated oxide, after being kept some time, appear to contract in some way, so as to take up less space in the bottle. It is not probable that this preparation undergoes any change through the agency of the atmospheric oxygen, the iron being already at its maximum of oxydation; hence, the most accurate exclusion of the air will not prevent its deterioration, and this must be attributed to another cause. Orfila has stated, in a note to the Academy, that colcothar, which is an anhydrous peroxide of iron, possesses no antidotal power, that is to say, it does not combine with arsenious acid. He says that if two mille-

grammes (.3 of a troy grain) of arsenious acid be boiled for two hours with water in which sixteen grammes (247 grains) of colcothar is suspended, the filtered liquid will still afford arsenic by means of Marsh's apparatus, and hence that 833 times the weight of the acid will not neutralize it.

Is it not probable that the true reason of the deterioration of the preparation will be found in a combination of the oxide and water when recent, which does not exist in that which has been long kept? That the presence of water is a condition of its activity, has been shown by the experiment of Orfila, quoted above. To ascertain, if possible, whether the hydrate long kept, (under water,) had the same amount of water in its composition as that recently precipitated, four specimens were taken from those previously used—the two first, four and two years old, the two last, nine months old and recent. They were dried at a temperature of from 80° to 90° Fahr., for twelve hours, a given quantity of each was introduced into glass bulbs afterwards balanced, and then subjected to a red heat, until they ceased to lose weight.

The result showed that the recently precipitated oxide contained nearly double the amount of water in the two first, and more than in the third. Whether the *proportion* of water combined with the oxide is a condition of its activity or not, the above statement, taking either view of the subject, is a curious coincidence; the proportion of water being very nearly that of their activity, as seen by reference to the previous experiment.

Orfila further says in the paper before alluded to, "I have proven by numerous experiments, that if the hydrated peroxide, instead of being in the state of *magma*, be used dry, that is to say, hydrated but not moist, and at a temperature of 36° to 40° centigrade, 16 grammes will neutralize about six decigrammes (9.26 grs. troy) of arsenious acid. At least the aqueous liquid, resting over 16 grammes of the hydrated oxide, which had previously contained 6 decigrammes of arsenious acid for some hours, did not become yellow on the addition of a solution of hydro-sulphuric acid, to which a few drops of hydro-chloric acid had been added." From these remarks it is evivdent that the dry oxide is not so active as that recently precipitated, as less than half the quantity of the latter removed the same proportion of arsenious acid from solution in about five minutes; indeed, it must be evident, that in a case where such feeble affinity is exerted, so great a change in the state of aggregation of a substance like hydrated oxide of iron, as is produced by drying it, and afterwards triturating it to powder, would materially interfere with combination. To test the matter, however, the following experiments were made, viz.: Sixty grains of recently precipitated oxide, dried at a temperature of from 80 to 90° Fahr., were mixed with water and three grains of arsenious acid in solution added, and the mixture occasionally shaken. In half an hour the arsenic was removed. Sixty grains of a dry hydrate, six weeks old, had not removed, in seven hours, three grains of arsenic; but 120 grains of the same oxide separated the same quantity in about four hours. Sixty grains of a specimen of hydrated oxide, made a year or more, had not removed the same quantity of arsenic when examined in twenty-four hours; 120 grains of the same oxide removed the arsenic in about ten hours.

From these observations it is clear that *quantity* may, to a certain extent, make up for *quality*. They also render it probable that if a larger proportion of the specimens of greatest age in the first series of experiments had been employed, that they would have removed the arsenic sooner, but this

does not do away with the evidence of those experiments, viz. that hydrated peroxide of iron deteriorates by age, etc.*

Orfila again observes, " MM. Nonat, Deville and Sandras, have advised, and with reason, to use in preference the dry hydrated peroxide, because it it contains in the same weight four times the amount that it does in the state of magma ; and they also advise to give 16 grammes of the hydrated peroxide for each grain of the arsenious acid to be neutralized." It cannot be doubted that the dry hydrated oxide can be administered in less space ; but at the same time it is equally true that the recently precipitated oxide is much more active, and should always be employed in preference, and besides the recent article from its levity would remain perfectly suspended in the fluids of the stomach and be active at every point.

Method of preparing the hydrated peroxide.—Dr. Fisher recommends that this preparation be made directly from the metal, by first forming a sulphate of the protoxide and then per-oxidizing by nitric acid, precipitating by ammonia and washing. This course will always give a pure preparation, (if the iron is good,) but the length of time it takes is an objection to its adoption, at least in a case of urgency. There are few pharmaceutists but have in their possession a sulphate of iron pure enough for making this preparation. By employing the sulphate ready made, the time of making is reduced one-third.

Take of Crystallized Sulphate of Iron, in coarse powder, ℥xx. Sulphuric Acid, ℥ij. Nitric Acid, (sp. gr. 1.4) ℥iiss. Water, Oiss.

Add the sulphate of iron to the water, previously boiling in a suitable vessel, and when it is dissolved add the sulphuric acid. To the boiling solution the nitric acid is to be added in small quantities at a time, continuing the ebullition after each addition until the whole of the acid has been added and the solution has attained a deep reddish-brown color. The dark liquid thus obtained is a concentrated solution of the tersesquisulphate of iron, and forms a ready means of obtaining the hydrated peroxide.

Another method which has been proposed is to take any quantity of nitric acid, specific gravity 1.3, and add powdered sulphate of iron in small quantities at a time until effervescence ceases, then applying heat to drive off the absorbed deutoxide of nitrogen. The only objection to this process is that the resulting solution is a mixture of two equivalents of tersulphate, and one of ternitrate of the peroxide of iron, and when an alkali is employed to precipitate the oxide, an alkaline nitrate remains in solution which, if not wholly removed, would be more likely to irritate the stomach than the sulphate of the same base.

However prepared, the strength of the solution should be known. Every hundred grains of the crystallized sulphate of iron employed, yields nearly thirty-eight grains of the hydrated peroxide ; and by knowing the quantity of sulphate employed and of solution obtained, the per centage of oxide in the solution is easily found. As made by the first formula, provided the resulting solution measures two pints, each fluid ounce contains one hundred and fourteen grains of hydrated peroxide. This solution should be kept in every

* Since the above was written, six grains of arsenious acid in solution were introduced into a half pint bottle of the antidote, containing about six drachms of the hydrated peroxide which had been prepared two years. Three weeks afterwards the filtered solution was strongly charged with the arsenic, notwithstanding the mixture had been frequently agitated.

shop as a source for obtaining the peroxide for antidotal purposes, or for any of the preparations of that oxide called for in the course of business. Having this solution at hand, supposing a sudden demand for the antidote occurs, the pharmaceutist should form a judgment as to the quantity of oxide wanted, and take a corresponding amount of the ferruginous solution, mix it with its weight of water, and add a very slight excess of solution of ammonia to precipitate the oxide. The whole should be thrown on a coarse flannel, and by gradual pressure as much as possible of the fluid be removed. By again adding water, and compressing, the oxide is obtained sufficiently washed for the first exhibition, so that no time be lost, and the remainder should be treated more completely, yet as rapidly as possible, so as to continue its administration at short intervals. By this course, the first doses may be administered in ten or fifteen minutes, if the manipulator has exercised ordinary skill. It is, of course, to be understood that the peroxide should be kept constantly on hand by every apothecary. After having washed it he should introduce it into bottles, in the form of a thick magma, so that after standing even for several months there should be little separation of water. This, however long kept, or even the dry hydrated peroxide (the precipitated carbonate of the U. S. P.) should be at once administered while the recent is making, and thus save time, but the importance of having the recently prepared oxide, particularly where the amount of poison taken is large, cannot be doubted.

In the case of the Gigon family, in which nine persons were poisoned by arsenic, reported by Drs. Smiley and Wallace in the Medical Examiner,* the writer had an opportunity of putting the above mentioned process in practice, and furnished the oxide to the patients in twenty minutes after being advertised of the demand.

In conclusion we may observe,

1st. That hydrated peroxide of iron, even when kept under water, gradually decreases in its power of neutralizing arsenious acid.

2d. That if kept in the form of a thick magma, it will retain its properties longer than when mixed with much water.

3d. That this decrease in power is probably due to a change in the relative proportion of the oxide, and the water chemically combined with it, as well as to an alteration in its state of aggregation.

4th. That from the experiments of Orfila, and others, the dry hydrated oxide possesses the power to a considerable extent of neutralizing arsenious acid, and it should be used in the absence of the moist and recent preparation.

5th. That hydrated peroxide of iron may be obtained in a state fit for use in ten or fifteen minutes, by using a solution of the persulphate of iron. And,

Lastly. That the recent oxide should be used in all cases where it is attainable, in preference to that long kept.

[It is well known that gelatine, especially in the form of calves' feet jelly, is often prescribed as an appropriate article of food for convalescents. We have often doubted the propriety of this advice, especially if the stomach of

* Vol. iii, p. 679.

the patient be still feeble, for the digestion of this substance certainly requires more active powers than that of broths and other highly animalized preparations. Combined with other articles of food, it answers a good purpose, but not when given alone. The results of the French experiments seem to be simply these, that the bone or gelatine soup does contribute to nourishment if combined with flesh, but, given alone, it is a starvation diet.]

Report of the French Academy on the Nutritive Properties of Gelatine.—The committee, consisting of MM. Thenard, D'Arcet, Dumas, Flourens, Serres, Breschet, and Magendie, fed a number of dogs with gelatine, in its various states, and as obtained from various substances. They found that these animals, in general, obstinately refused to take it above once or twice, and died as speedily as if they had been completely deprived of food. Even when the gelatine was flavoured, by being boiled with a little fresh meat, though the dogs partook of it more readily at first, all died in the time they would have done, had they been deprived altogether of nourishment. When fed on a mixed diet, the dogs were perfectly nourished, and even when fed for months on raw bones, maintained their health and vigour unimpaired. Bones which had been boiled had not, however, the same nourishing qualities; the animals rapidly fell off when kept on that diet. The conclusions at which the committee arrived were the following:—

1. No known process can extract from bones an aliment which can supply the place of fresh meat.

2. Gelatine, albumen, or fibrine, taken separately, only nourish an animal for a very limited period, and in a very imperfect manner. In general, these substances excite in a short time an insurmountable disgust, so that the animals will rather die of hunger than eat them.

3. These same principles, artificially united, and rendered savoury by proper seasoning, are taken more freely and for a longer time than when separate, but, in the long run, they nourish the body no better; for the animals which eat them, even in considerable quantities, end by dying with all the signs of complete starvation.

4. Muscular flesh, in which gelatine, albumen, and fibrine are united according to the laws of nature, and are associated with other matters, as fat, salts, &c. suffice, even in small quantity, for complete and prolonged nutrition.

5. Raw bones have the same property, but the quantity consumed in twenty-four hours requires to be considerably greater than if fresh meat were given.

6. All kind of preparation, as decoction in water, the action of hydrochloric acid, and, above all, its transformation into gelatine, diminishes the nutritive quality of bones, and seems even in certain cases to deprive them of it altogether.

7. The committee do not take on them at present to decide as to the question of the nutritive powers of gelatine for human food when given along with other alimentary matters; they are aware that direct experiments on a large scale can only decide the question. They are engaged in this investigation at present, and will make known the results soon.

8. Gluten, as procured from the flour of wheat or of maize, suffices of itself for a complete and prolonged nutrition.

9. Fat substances, taken as the only aliment, support life for a long time, but they give rise to an imperfect and disordered nutrition, where fat accu-

mulates in all the tissues, sometimes in the state of oleine and of stearine, sometimes in the state of almost pure stearine.—*Ed. Med. and Surg. Jour.*, from *Comptes Rendus des Seances de l'Academie des Sciences.* August, 1841.

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On the employment of the Gelatine and Fat of Bones as a means of ameliorating the diet of the poor. By M. D'Arcet.—M. D'Arcet, from the examination of the annual reports of various hospitals, gives an apparently satisfactory answer to the seventh conclusion of the French Academy as to the nutritive power of gelatine. He shows, that the directors of the different hospitals at Lille, Metz, Lyons, Strasbourg, different parts of Russia, and the whole of Holland, have found experimentally, that the health of their inmates has improved from the date of their fitting up an apparatus for extracting the gelatine and fat from bones. With regard to one of the hospitals at Lyons, he mentions the very striking fact, that the mortality appeared to be reduced by this means alone from 90 to 72, at which it has averaged ever since the apparatus for the preparation of the gelatine soup was fitted up. Many other interesting facts are mentioned, showing the advantage of hospitals being provided with an apparatus for the preparation of gelatine from bones, for it appears that, whenever this gelatinous soup has been added to the diet of the patient, the sickness, and consequent expense for medicines has been greatly lessened, the mortality has been considerably diminished, and the general health of the inmates greatly improved.—*Ibid.*, from *Annales D'Hygiene Publique.* April, 1841.

On the Ferruginous Bread of M. Derouet-Boissière. By the commission of the French Academy.—M. Derouet-Boissière having been extremely successful in curing obstinate cases of amenorrhœa and chlorosis, which had resisted all other means of treatment, by means of bread impregnated with minute doses of iron, a committee of the Academy of Medicine was appointed to report on the subject. They accordidgly saw bread prepared with small quantities of lactate, acetate, and protocarbonate of iron, and proceeded in the public hospitals to make trial of its powers. The bread was prepared in the proportion of 3½ ounces of bread to 3 or 4 grains of the salts of iron, and it is stated that when the iron did not exceed this quantity, the bread had no disagreeable flavour, though the chalybeate taste was slightly apparent.

During the months of November and December, 1840, seven chlorotic patients were treated with this bread in the Hotel Dieu. They were allowed two small loaves of this bread daily, each containing about three grains of the lactate of iron, and occasionally an equal quantity of sub-carbonate. Of the seven chlorotic females, five had the disease in a very advanced stage, the other two were in a state of anemia, brought on by repeated and copious evacuations of blood. These last two were not able to take the quantity of bread allowed them at first, but gradually became able to do so, and left the hospital cured, the one after six weeks of treatment, the other after two months. Their menstrual discharges were by that time completely re-established.

Two of the confirmed cases of chlorosis were sensibly improved after the use of the bread for six days, and one was dismissed cured in a fortnight, the other in a month.

The other three cases were old confirmed cases, which had resisted the

usual treatment employed for the cure of this disease, and their cases are given in detail. In the first of these cases the treatment by the ferruginous bread was begun on the 20th of November, and by the 6th of December the menstrual secretion appeared and continued for three days. She was nevertheless kept in the hospital for another month, when, having become quite regular, and all symptoms of chlorosis having disappeared, she was dismissed. In the second case the ferruginous bread was begun to be administered on the 4th of December. On the 10th she was less pale and felt better. On the 24th the menses appeared and continued four days. She rapidly improved from this date, but was detained in the hospital till the next menstrual period was over, when she was dismissed cured. The third case was the most severe, and the symptoms of chlorosis of longest duration; the health seemed completely broken. The treatment commenced on the 20th of December, and she was allowed two of the small loaves daily. For the first fifteen days there was little apparent improvement, but from this date the symptoms of amendment became obvious. Her pallid cheeks gradually acquired a little colour, she improved in health and strength, and was dismissed with all the marks of health on the 22d of February.

Soon after this chalybeate bread was administered, the committee were able to detect the presence of iron in the urine, but its appearance there seemed to be inconstant, as they would often detect it at one time, and not at another.

The committee from these trials concluded, that this is an excellent mode of administering chalybeates; that, when in the small proportion of two or three grains of this salt to the loaf of three ounces and a half, its presence cannot be detected by any change of colour; that the salt of iron used is not decomposed by the process of baking; and that the iron can always be detected in it by the use of the ordinary reagents.

It may be remarked, that M. Derouet-Boissière was first led to administer iron along with the food, from the conviction that it would be more likely to be taken into the circulation, by being absorbed along with the chyle, than if given according to the usual methods.—*Ibid.*, from *Bulletin de l'Academie Royale de Medecine.* April, 1841.

Case in which diarrhœa from ulceration of intestinal glands disappeared under the internal use of chloride of sodium. By Mr. C. J. Mill, Surgeon, Kirriemuir.—A. F., aged 29, after exposure to cold and wet, was seized with rigors followed by acute pain in the head and abdomen, and violent purging. He stated that for these symptoms he had lost a few ounces of blood, and taken calomel and salts. I saw him after he had been a fortnight confined to bed, and found him then very much reduced, almost unable to sit up in bed; with pulse at 90, full and hard; the abdomen tender on pressure or motion; purging almost incessant; matters passed sometimes bloody mucus in large quantities, at other times very dark thin feculent matter, like fluid pitch, always excessively fœtid. He complained much of tormina; also of irritation of urinary organs. He had great thirst; the tongue was glazed, red, and dry; total loathing of food. He complained still of pain in the head; and last night he was a little incoherent.

I opened a vein and bled him to approaching syncope; gave calomel, castor oil, and opium; had the head shaved, and cold lotions applied; blistered the abdomen, and then applied hot poultices. Arrow-root gruel was prescribed for food.

After bleeding, &c. the pain in abdomen was much relieved, but the purging continued unabated. The matters discharged were still as dark coloured (not such a colour as arises from the use of mercurials,) and fœtid as ever. The tongue and teeth were covered with black sordes; the pulse was quick, small and weak; the patient had low muttering delirium; but the pain in the abdomen was gone. As the purging was giving so very great uneasiness to the patient whom I considered sinking from sphacelus, the result of neglected inflammation of the mucous membrane of the bowels during typhoid fever, I ordered chalk mixture to be given every three hours until it should abate; and gave wine and nutritious soups. These measures were followed by relief at the time; but whenever the chalk mixture was intermitted, the tormina and purging recurred as before.

The fœtor from the evacuations and breath was now so excessive that I gave some solution of chloride of sodium to sprinkle in and about the patient's bed to quench the smell. It occurred to me, that, if given internally, it might act in a similar way by correcting fœtor, and inducing healthy action. I therefore ordered a drachm of it to be given in half an ounce of chalk mixture, and repeated every four hours. Next day when I visited my patient, I was quite delighted and surprised to find his appearance so much changed for the better. He had had a refreshing night's sleep; the purging was abated; the tormina ceased; there was no fœtor; and the black crust was separating at the edge of the tongue. The countenance was expressive of hope. He had taken a little arrow-root in the morning. I ordered the chloride to be continued, omitting the chalk mixture, and substituting wine in its place. His recovery has been rapid, and is now complete; only requiring an occasional small dose of castor oil.—*Ibid.*

Cramp in the Stomach. By Robert Graham, Surgeon.—About eight or nine years since I had a patient in Glasgow, a married lady, about thirty years of age. She had had a large family previous to my acquaintance with her, but had for many years been subject to violent attacks of cramp in the stomach; on account of one of which I was first called to see her, and at which time I thought she would have died. I need not enumerate the antispasmodics which were used at that time; having then and subsequently tried all those recommended for the complaint, without being able to say that I had succeeded in even checking the spasm for the time. It seemed eventually to wear off of itself. I had bled her, which gave relief for once; but it was followed by such weakness, that when called to witness another attack two or three days after, I dared not repeat it.

The thought, by and by, crossed my mind, that I could produce a counter-spasm; so I took a strong tumbler, and with a bit of lighted paper applied it as a cupping glass over the stomach, when almost immediately I had the satisfaction of hearing my patient, who could not speak a moment before, exclaim, "The pain is gone." Since that time it has invariably been a source of relief with her when attacked; and I do not recollect of its ever failing, if a large cupping glass was applied firmly once or twice over the part.—*London Lancet.* March 12th, 1842.

Undiluted Creasote in Burns. By John Sutherland, M.D.—It has been my intention for some time to send you a short communication on the use of undiluted creasote in burns, but I have delayed doing so in the expectation that I might be able to try the remedy in a greater number and variety of cases, so as to obtain an estimate of its real value. In the 22d number of the Lancet, however, there is a communication from Dr. Mitchell, of Dub-

lin, on the subject, in which the author requests information from other members of the profession who may have used it, and in consequence of this I shall state shortly the result of my experience.

About two years ago I accidentally dropped some burning sealing-wax on my hand; I removed it as quickly as possible, and applied to the surface a drop of creasote from a bottle which happened to be beside me; it afforded immediate relief, and in less than five minutes the pain was gone. No sensible vesication took place, but in a few days a cuticle peeled off, and left a dry, reddish surface beneath. A short time after this one of my servants burned the palm of her hand by laying hold of a hot iron; in a few minutes creasote removed the pain, and I heard nothing more of it. I have used it in ten or a dozen similar cases, and I have always found it remove the pain in a very short time. The effects I had observed induced me to bring the subject before the Medical Institution of Liverpool (as I find by the minutes) on the 1st of April, 1841, in the hope that such members as had opportunity might give the remedy a trial. I have had no experience of its use in severe injuries, and on this point I can bear no testimony, but in the common domestic burn or scald it has appeared to me so useful that I have recommended its being kept in families for the purpose of immediate application.—*Ibid.*

Asphyxia dissipated by a pinch of Snuff.—A child, five months old, being constipated, was given a spoonful of sweet oil. The dose having been carelessly administered, some of the oil got down the teachea, and brought on complete asphyxia; the lungs had ceased to contract, and the infant was on the point of death. M. Pigeau, who was sent for, at once perceived the cause of this alarming state, and passed a pinch of snuff up the nostrils of the infant. The violent sneezing thus brought on excited the contractibility of the respiratory muscles, and respiration commenced, feebly at first, but was gradually re-established. Strong vinegar and other means had been previously tried, but without without success.—*Provincial Med. and Surg. Journal.* March 12th, 1842.

Croup in the Adult.—At the Royal Academy of Medicine, March 8th, M. Huguier presented the larynx, &c. of a female who had died of croup. The woman was 24 years of age, and the disease was unaccompanied by the cough peculiar to croup; the only symptoms present were aphonia and the hissing respiratory sound. The patient died suddenly in forty hours from the commencement of the attack, without any signs of asphyxia, suffocation, or lividity of the countenance.

On examination, after death, false membranes were found lining the amygdalæ, the pharynx, larynx, trachea, and upper divisions of the bronchi. The right cavities of the heart contained fibrous clots, which adhered firmly to the walls of the heart, and sent off various prolongations between the carneæ columnæ and into the pulmonary artery. The author seems inclined to attribute the sudden death of the patient to the coagulation of blood in the heart.—*Ibid.* March 19th, 1842.

Experimental Researches on the Function of the Skin in man and animals. By Dr. Ducros.—In a very curious experimental paper, Dr. Ducros shows that a coating of gum-lac put on the skins of animals causes them to die in a longer or shorter time, by producing convulsive movements similar to epilepsy. When the animals, coated with gum-lac, were subjected to electricity, they died in a much shorter time. He next tried the effect of metallic coverings, as he entertained the notion that, because they had oppo-

site electrical properties, the animals coated with them would die with symptoms of an opposite nature. He therefore cut off the hair from some animals and covered them with thin plates of tin, (tin-foil) and found that they perished with symptoms of debility, the reverse of what he had noticed when the coating consisted of a resinous substance. When the tin was covered with a coating of gum-lac, the animals perished still more rapidly. He then placed under the influence of electricity some of the animals covered with plates of tin, and found that so long as they remained connected with the electrical current, their vigour appeared to be restored, but that, whenever it was arrested, they appeared ready to perish.

The object of these experiments was to ascertain what would be the likely effect of such coverings in certain diseased states of the human frame, and especially in nervous or neuralgic affections, and in rheumatism. He reasoned, that, if metallic coverings deprived animals of life by producing rapid sinking of the vital powers, the same metallic plates applied to the human body would cure or remove those diseases which seemed to depend on an excess of organic life. On putting his plan to the test of practice, he was so fortunate as to find that it removed some nervous, and a few acute and chronic rheumatic affections.

This plan of treatment was of no avail in any case where the disease was dependant on or connected with organic lesions, or attended with fever, or swelling of the part, or with general weakness; on the contrary, in all these cases the metallic plates augmented the disorder.—*Ed. Med. and Surg. Jour.*, from *Comptes Rend. des Sean. de l'Acad. des Sci.*, 20th Sept., 1841.

Hæmorrhage after Lithotomy.—At the Academy of Medicine, Paris, M. Begin read a memoir on the hæmorrhage which occurs after the operation of lithotomy. If we consider the normal anatomy only (says the author) of the parts concerned in the operation of lithotomy, we shall have a very imperfect idea of the cause and frequency of this accident. In many cases the occurrence of hæmorrhage depends on various anomalies in the course and volume of the vessels, and on their dilatation; but in others the bleeding takes place by a sort of exhalation, without the injury of any considerable vessel.

The statistics of lithotomy show, that one out of every five or six patients, who are cut for the stone, die; it would be highly useful if we would determine in what proportion the different accidents that attend the operation contribute to this mortality. This is a very difficult question, and all that the author attempts to do is, to affirm generally that about one-fourth of the total deaths depend upon hæmorrhage. Authors lay much stress on the necessity of determining the exact seat of the bleeding, and they give several rules on this point; but M. Begin observes, that it is always very difficult to form any precise idea of the vessel from which the bleeding comes. The author next passes to an examination of the various means employed to arrest the hæmorrhage—viz. pressure, ligature, plugging, cauterisation, cold injections, &c. &c.; he affirms that none of these means can be confidently relied on in all cases. He then mentions a process which he has adopted with success in three cases where the bleeding had resisted every other means. In the first case he had the patient placed on the edge of a bed, and made his pupils keep constantly injecting cold water into the wound. After the lapse of an hour the bleeding ceased, and did not return. This method, however, is extremely tedious and difficult of execution; the author, therefore, in a second case, substituted for it the keeping up a constant current of fluid, by means of a tube placed in a pail of cold water.—*Provincial Journal.* March 5th, 1842.